of the second decimal. Very concordant determinations of density gave as a mean number 19·90.

Argon, therefore, shows no sign of association on cooling, nor of dissociation on heating, as Prof. Bevan thinks it might.

RAYLEIGH.

Terrestrial Helium (?).

PROF. PASCHEN and I have lately made a careful determination of the wave-length of the strong yellow line emitted by cleveite when heated in a Plücker tube. We owe the mineral to the kindness of Prof. Rinne. My large Rowland concave grating of 6·5 metre radius, clearly shows the yellow line to be double. Its less refrangible component is much weaker, but comes out quite bright, when the stronger one is brilliant. We photographed the two lines together with the second order of the spark spectrum of iron. There are a number of iron lines on each side that are included in Rowland's list of standard wave-lengths (*Phil. Mag.*, July 1893). From these we interpolated the wave-lengths of the yellow lines by micrometric measurement. Three different plates taken on different days gave us :

Strong component.	Weak component.
5875·894	5876·216
5875·874	5876·206
5875·880	5876·196
Mean 5875·883	Mean 5876·206

We think an error of more than 0·025 very improbable.

Now Rowland's determination of D_3 (*Phil. Mag.*, July 1893) is :—

5875·982

the result of three series of measurements which he believes to be accurate to 0·02.

The difference between this value and the wave-length of the strong component is much too large to be accounted for by an error of observation.

We do not therefore agree with the conclusion, drawn by Mr. Crookes, that the unknown element helium causing the line D_3 to appear in the solar spectrum is identical with the gas in cleveite, *unless D_3 is shown to be double*. Perhaps Prof. Rowland will tell us if this might have escaped his notice. From his note on D_3 in *Phil. Mag.*, July 1893, it appears that D_3 cannot have been so wide as to include both lines, because he would then not have considered his determination accurate to 0·02. As for dispersion, one may see in his table of solar spectrum wave-lengths that he has frequently measured three and even four lines in an interval as large as the one between the components.

Hannover Techn. Hochschule, May 16. C. RUNGE.

The Origin of the Cultivated Cineraria.

I HAD hoped that it would not be necessary for me to say anything more upon this subject. But Mr. Bateson's last letter seems to require a few remarks on my part.

I confess that I find it very difficult to follow his train of arguments. All I can do is to restate once more my original position, and endeavour to see how far Mr. Bateson has been successful in impugning it. I am sorry that Mr. Bateson thinks I have "treated" him "to some hard words," though I confess he seems to me, in that matter, quite able to take care of himself.

I asserted then (*a*) that the cultivated Cineraria only differs from the wild form, putting colour changes aside, in dimensional differences. I believe that in saying this I am expressing the deliberate opinion of the Kew staff, the members of which, such is human nature, would have no hesitation in disagreeing with their chief, if they thought otherwise. To this point I do not understand that Mr. Bateson advances any serious objection.

Secondly (*b*) I asserted that these dimensional differences had been gradually accumulated. To this I understand Mr. Bateson demurs, though I fail to see that he has brought forward a particle of evidence to prove the contrary.

Now for Mr. Bateson's own position. He asserts, in common with other authorities, that the modern Cineraria is of hybrid origin. I have arrived at an opposite conclusion. And here I may quote the support of Dr. Masters, F.R.S., the well-known editor of the *Gardeners' Chronicle*, who in that paper for January 24, 1891, p. 108, states :—"Carnations and Picotees, again, which originate from one species, vary from seed but not from buds ; and the same may be said of the Cineraria, the offspring of one species."

Mr. Bateson complains that I do not give "any specific answer" to the historical evidence. I thought I had made it sufficiently clear in my last letter that: (*a*) I doubted its value for scientific purposes ; (*b*) I set it aside as irrelevant on account of the impossibility of proving the descent of the modern Cineraria from its supposed ancestors. Both Prof. Weldon and I have shown that the historical evidence can be handled both ways. But I prefer to set it aside altogether in the face of objective facts.

Mr. Bateson's next step is one to which I most seriously demur. He transforms a proposition of mine into terms to which I could not assent, and then proceeds to attack it. He makes me say that "to improve a plant the only safe way is by selecting," &c. I absolutely never said anything of the kind. "Improve" in horticulture is a word of large connotation. I confined myself to the production of dimensional changes, and I believe that what I said was in accordance with horticultural experience.

To demolish my position, Mr. Bateson has to get over the fact, which seems to me incontestable, that there is no essential morphological difference between the cultivated Cineraria and the wild *C. cruenta*. To do this he trots out the Himalayan rabbit. I cannot but admire his courage. What possible analogy can there be in the two cases? Two "breeds" of rabbits are crossed and produce a third *different from either*. If the modern Cineraria is of hybrid origin, then it has eliminated traces of all but one of its parents. The principle of economy of hypothesis makes me slow to believe this. Anyhow the Cineraria has clearly not produced anything analogous to a Himalayan rabbit which differs from both its parents.

As to Mr. Darwin's account of the origin of the Cineraria, I must frankly take the responsibility. I have no doubt he worked with ordinary garden kinds. He wrote to me for information as to their origin. At the time I was entirely ignorant of the subject. I wrote to Mr. Thomas Moore, who was considered the best authority on such matters, and he sent me the traditional account. I passed it on to Mr. Darwin, with the opinion, no doubt, that I thought the information trustworthy. So I am afraid Mr. Bateson is only appealing in this case from Philip sober to Philip drunk ; *i.e.* from my own considered opinion to my unconsidered one.

I will now wind up all I have to say on the subject with a few miscellaneous remarks.

There can be no two opinions as to the importance of the study, from the point of view of organic evolution, of the changes which can be brought about in plants under cultivation. But it must be conducted with scientific precision. This discussion will not have been fruitless if it directs attention to the subject. A beginning has already been made. M. Bornet has worked on the genus *Cistus* at Antibes, and has reconstructed some of the forms, as to the origin of which there was only "historical evidence," described and figured by Sweet. My friend Count Solms-Laubach is engaged on the cultivated forms of *Fuchsia*, and I am quite sure that any results he arrives at may be accepted with implicit confidence. As he has asked me for species of *Cineraria*, I hope he may look into this matter also.

I must repeat my caution as to the danger of accepting horticultural evidence as to hybridity. I will give a few recent instances. I could easily give a long list with chapter and verse for each.

(*a*) *Thuya filiformis* was long considered to be a hybrid between *Juniperus virginiana* and a *Thuya*. It is now known to be a "growth-stage" of *Thuya orientalis*. The history is discussed by Sir Joseph Hooker in the *Gardeners' Chronicle* for June 22, 1861, pp. 575, 576. It affords a delightful commentary on the hybridisation fallacy and the value of "historical evidence."

(*b*) Some years ago we received at Kew bulbs of what professed to be a hybrid between *Amaryllis Belladonna* and *Brunsvigia Josephinæ*. When it flowered, it was evident that it was no hybrid at all, but only a very fine form of the former species. This is rarely propagated from seed. In this particular case seminal variation had come into play with corresponding dimensional change. The hybrid origin is recorded in the *Gardeners' Chronicle* for September 4, 1875, p. 302. It will, no doubt, be dug out hereafter as "historical evidence."

(*c*) The last number of the *Gardeners' Chronicle* (June 1, 1895,

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p. 692) affords a striking instance. Hybrid Cypripediums are of considerable pecuniary value. One recently exhibited at the Royal Horticultural Society was at once denounced as no hybrid at all, but a merely seminal variation. The possessor has fears that it will "add one more to the long list of doubtful crosses by which auctioneer and purchaser are alike misled."

Notwithstanding the Himalayan rabbit, I am afraid botanists will continue to refuse to accept hybrid origin on historical evidence unless there is palpable objective proof of the fact.

There are two additional bits of evidence, to which, however, I do not attach great weight, but which may be recorded to complete the story. It is, at any rate, agreed that the Cineraria originated from the Canaries. I have already pointed out that De Candolle divided the wild Canarian species into shrubby and herbaceous. I do not believe that they are mixed in the modern Cineraria, which remains entirely herbaceous. Now, Schultz-Bipontinus, who described the Canarian species for Webb and Berthelot, relegates the shrubby species to *Senecio*, and the herbaceous to *Doronicum*. Though this is not now sustained, it shows that the two groups are not very closely related, and diminishes the probability of their freely intercrossing.

On the other hand, *Cineraria cruenta* and the modern *Cineraria* cross with the greatest facility. In fact, if you grow the two together it is almost impossible to keep the wild species true. I have no doubt that in a short time we shall be able to combine the pleasing habit of the wild plant with the fine colour of the modern strains. All this does not surprise one, as to me they are all essentially the same thing.

I must add one word more. I cannot but think that there is a growing danger nowadays of a pseudo-biology growing up for the especial use of evolutionists. This is not the first time by many that I have been so unlucky as to come into collision with it. Long ago I pointed out in these pages that biology is not a deductive science, and for the present, at any rate, theory must be adjusted to facts, not facts to theory.

W. T. Thiselton-Dyer.

Royal Gardens, Kew, June 1.

Mr. Bateson now admits that *some* named varieties of *Cineraria* may have arisen from pure-bred *C. cruenta*, or from plants believed to be pure-bred. He holds that these have become extinct, while Mr. Dyer believes the hybrids to have disappeared. I have never attempted to discuss this question, and shall not do so now. I wish only to justify my interpretation of the passages I quoted against Mr. Bateson :—

(1) Mrs. Loudon begins the article quoted by both of us with these words : "Most of the purple *Cinerarias* are varieties, or hybrids, of *C. cruenta*." She then goes on to say that in or about 1827 (the year in which he recommended the growth of pure-bred *C. cruenta* "for the production of fine double and single varieties"), Drummond, of Cork, produced certain hybrids ; while since his time other hybrids had been made. She then, in a new paragraph, says : "Some of the most beautiful *Cinerarias* now in our greenhouses have been raised by Messrs. Henderson . . . particularly *C. Hendersonii* and the King, both raised from seeds of *C. cruenta*" ; and a line or two further : "Two new ones have lately been raised, of remarkably clear and brilliant colours, apparently from *C. cruenta*, named Queen Victoria and Prince Albert," &c.

It will be seen that the general statement, with which the article begins, declares "*most purple Cinerarias*" to be "*either varieties or hybrids*" of *C. cruenta*. Of others, and of those Cinerarias (such as "the King") which are not purple, nothing is said. This general statement is illustrated by examples, first of hybrids, next of pure-bred varieties.

In discussing the examples of pure-bred forms, Mr. Bateson omits to notice "Queen Victoria" and "Prince Albert," and discusses only *Hendersonii* and "the King." He believes Mrs. Loudon, in saying that these were "raised from seeds of *C. cruenta*," to mean simply that *C. cruenta* was the female parent, the male being unknown, or unnamed. I do not know what degree of inaccuracy Mr. Bateson is willing to attribute to Mrs. Loudon ; but in the writings of serious botanists a "seed" means the fertilised product of two elements, the ovule and the pollen grain ; and therefore the "seed" of *C. cruenta* means the product of two parents, both of which belonged to this species.

Mr. Bateson says that six or seven years after writing the passage in question, Mrs. Loudon speaks of *C. Hendersonii* and the King as "hybrids." This simply shows that she changed her mind ; and although it may affect the value of her opinion as evidence, it does not alter the plain meaning of her words in 1842.

(2) The only author whom I quoted as asserting the pure-bred origin of *C. Hendersonii* and the King was Mrs. Loudon. It is true that in two other articles quoted by Mr. Bateson these plants are called hybrids. I did not allude to this matter in my first letter, because I hoped Mr. Bateson would himself see the folly of attributing to these articles any definite meaning whatever. It will suffice to consider one of them.

In the earlier article, describing *C. Waterhousiana* (*Paxton's Mag. Bot.* iv. p. 219), that plant itself is called a "variety," although it is said to be the offspring of specifically distinct parents. On p. 221, *C. Hendersonii* is alluded to in these words: "The following are the names of some of the hybrids raised and cultivated by Messrs. Henderson . . . *C. cruenta var. Hendersonii*, *formosa*, &c." Both these passages are meaningless, if the words "hybrid" and "variety" are construed strictly. If they are not to be so construed, and they evidently cannot be, then I was justified in ignoring the passages, for they prove nothing but the incompetence of their author.

On the other hand, the passage which I did quote from this article is at least intelligible ; and it asserts that *C. cruenta* "may be regarded as *the parent*"—which means, if it means anything, the *only* parent—"of many of those beautiful varieties so successfuly cultivated by Messrs. Henderson," &c. This passage Mr. Bateson does not consider in his reply to me.

The second article (*Paxton's Mag.*, 1842, p. 125) in which the King is called a hybrid, uses the word in the same loose fashion, and it would be as easy as unprofitable to quote other passages in which the same plants are called now "varieties" and now "hybrids."

Enough has been said to show that Mr. Bateson's original evidence does in fact bear the interpretation I put upon it ; and further, that the words "variety" and "hybrid" are so loosely employed by early writers that their records are often of little value. Stories of hybridism and sporting are frequently brought forward on such evidence ; so that I have thought it worth while to examine the case for one such story, as stated by its advocate. Having done this, my interest in the matter ends, and I do not propose to speak further upon it.

W. F. R. Weldon.

University College, London, May 31.

Some Bibliological Discoveries in Terrestrial Magnetism.

In a letter on the above subject, by Dr. L. G. Bauer, published in Nature of May 23 last, I read as follows : "I find it asserted that the Frenchman, L. J. Duperrey, was the first (1836) to construct 'magnetic meridians' for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle." The writer then remarks that the honour of first introducing this method is due to Thomas Yeates, an Englishman, in 1817.

This is hardly correct, as I possess a coloured map of the Northern Hemisphere with the "magnetic meridians" as described shown upon it of an earlier date. The title of the map is :

"To George Washington,
"President of the United States of America,
"This Magnetic Atlas or Variation Chart is humbly inscribed by John Churchman."

As Washington died in December 1799, it is evident that John Churchman has a prior claim to being the first to construct "magnetic meridians."

Ettrick W. Creak.

London, May 31.

Effects of Earthquake in Sumatra.

On May 17, 1892, an unusually severe earthquake was felt through nearly the whole of North Sumatra ; most severely shaken was the district between the Dolok Lubuk Raja and the Gunung Talamau (Ophir). Serious landslips occurred in many parts of the mountains, especially near the summit and along the slopes of the Gunung Merapi, a volcano 2145 metres high in the residency Tapanuli. On inspection it was found that the safety of a brick pillar, erected on its most elevated point by the triangulation service, was endangered by part of the crater having been

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